

In our view, and contrary to that of Drs T. Ziegler and W. Böhme (BZN 55: 112), the ability to use stable nomenclature for the inclusion of species and subspecies in CITES and other conservation legislative documentation is an important issue. Taxonomists are the servants of all those who use scientific names and work to serve those needs, not to establish an authority to which everyone must subscribe whether in accord with stability or not. We believe that our aim must be to provide an environment of nomenclatural stability in which biologists may work with confidence.

Additional reference

Rehák, I. & Velenský, P. 1997. Biology of the varanids *Varanus prasinus*, *V. rudicollis* and *V. salvadorii* in captivity. *Gazella*, **24**: 108–138. [In Czech; English summary].

Comment on the proposed suppression of all prior usages of generic and specific names of birds (Aves) by John Gould and others conventionally accepted as published in the *Proceedings of the Zoological Society of London* (Case 3044; see BZN 54: 172–182; 55: 176–185)

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We are the authors of the original paper under consideration as Case 3044. Various points covered by Schodde & Bock (1997), the comments of Olson (1998) and the response of Schodde & Bock (1998) [as cited above] require further comment. It should also be noted that our paper, although dated 1990, was published in 1991, as pointed out by McAllan (1992).

1. Inconsistencies in the use of reports published in *The Athenaeum*, *The Literary Gazette* and *The Analyst* prompted our review of these serials. The first two were of considerable importance for many years as general sources of information covering the sciences and other fields. The third was a short-lived journal from the 1830s and one of several from this period affecting zoological nomenclature. As an example of inconsistency, we pointed out that although *The Athenaeum* is accepted for *Balaeniceps rex* (a very brief but adequate description) in a standard work (Kahl, 1979), there were other names variously mentioned or overlooked, with equal claims to priority. Also, we deplored the proposal for suppression of a name from *The Literary Gazette* without the actual reference being examined (LeCroy, 1988; LeCroy & Bock, 1989), an action invalid for other reasons, as we discussed (Bruce & McAllan, 1991).

2. The latter example prompted us to provide verbatim extracts of the relevant references in our paper to facilitate an evaluation of our findings and to avoid the argument of the rarity or inaccessibility of the sources (a pointless criticism in view of the rarity and inaccessibility of many sources long accepted in avian nomenclature). We found hundreds of nomina nuda in our investigations but only discussed those names identifiable by descriptive details. For example, we did not discuss *D[inornis]*, *dromaeoides* because it is a nomen nudum in *The Literary Gazette*. The only nomen nudum we did discuss was *Sitta ferrugineoventris* in *The Athenaeum*

because Hartert & Steinbacher (1932) accepted it as an available synonym of *S. castanea*. As to the other names, these were interpreted under the application of the 3rd Edition of the Code to the status of the names at their time of publication last century. For example, *Chrysococcyx minutillus* was indeed the smallest cuckoo of this group known at the time, and the *Proceedings of the Zoological Society of London* [PZS] reference also stated this point (Gould, 1859).

3. We also covered mammals in the same format as birds (McAllan & Bruce, 1990). So far, there has been no attempt at blanket suppression of our findings, probably because the catalogue of Australian mammals already had been published (Walton, 1988). In fact, some of our findings have been used in major reference works (Corbet & Hill, 1992; Wilson & Reeder, 1993).

4. Olson's example of *The Zoologist* as another possible source of earlier publication of names is a valid point and needs further investigation. At the time we chose to exclude from our study long-running natural history serials well known to specialists of the period, e.g. *Annals and Magazine of Natural History*. A more important point is that if we extended our research to daily newspapers, we may find further earlier dates of publication of many more names. For example, Sulloway (1982) cited a report of a Zoological Society meeting from 1837 in three dailies (*Morning Herald*, *Morning Chronicle*, *Standard*) before its appearance in *The Athenaeum*. Newspapers often have been used as the original references of avian names, e.g. *The Sydney Morning Herald* (*Trichoglossus* [= *Charmosyna*] *amabilis* — Mayr, 1945; see also Watling, 1982); *The Kentucky Gazette* (*Chlidonias* — Rhoads, 1912; see also Peters, 1934); of 37 names proposed by Wilhelm Blasius, 20 first appeared in a local newspaper, *Braunschweigische Anzeigen*, and it is accepted as the original publication source in standard references (cf. Hinkelmann & Heinze, 1990); as well as various Australian examples (Whitley, 1938, as indicated by Schodde & Bock — see also Whittell, 1954, e.g. under Diggles, Ramsay; and Ingram, 1990 for De Vis). Indeed, given the number of Australasian taxa named in newspapers, we are amazed that Schodde & Bock had any problem with our findings at all.

5. As Schodde & Bock pointed out, we were present at the SCON meeting in Vienna in August 1994. In considering the issue of suppression we voted neither for nor against. We assumed that at least one of us, a member of SCON (MDB), would see a draft of the proposed submission for comment prior to any publication in the *Bulletin of Zoological Nomenclature*, or at least receive advice that it was to be submitted. Knowing that several years may elapse between proposal and submission, we were surprised to see it appear in the *Bulletin* in 1997 in a form where any input from us had been denied.

6. We regarded our paper as a forum for further assessment of our findings and expected some of our conclusions to be revised. We summarised our interpretations in an appendix and indicated where suppression seemed appropriate. However, no action had been taken by us on these points as we awaited further discussion of our paper and also intended to expand our investigations on related issues in other publications, particularly that of newspapers as sources of names.

7. We did not expect BZN to be the forum for discussion. Olson's interpretations have clarified some of our findings with consequent ad hoc changes to the original proposal by Schodde & Bock. These changes demonstrate our point that further revision of our findings was needed, not total suppression as a quick solution.

8. The proposal for blanket suppression is obviously because of concerns by the senior author, R. Schodde, to avoid considering the possible effects of our findings on Australian birds. We assume that the findings of Olson requiring Schodde & Bock to emend their original proposal are because they do not affect Australian birds. In stark contrast to this approach, compare how such issues affecting North American birds are handled. For example, Banks & Browning (1995) discussed a number of cases, including at least two where suppression is required. Their findings indicate that Oberholser (1974) is the chief source requiring their attention. These are all dealt with on a case by case basis. We assume that if Oberholser's publication had been on Australian birds, the entire work would have been submitted to the Commission for blanket suppression.

9. The motive behind the submission for suppression seems to be more concerned with changes to original citations and dates than with the issue of nomenclature. On the one hand, Schodde & Bock credit 'any zoologist with a knowledge of the alphabet' as being able to handle the growing subsidiary literature of suppression of names, yet also patronise them as being endlessly confused if our findings were to be absorbed into the literature. Are we to assume that Australian zoologists in particular are more prone to confusion than others?

10. Schodde & Bock are also concerned about changes to original citations of avian names as they appear in standard references, many now out of date (original citations and standard references). Such changes have always been a very small proportion of the total, e.g. North American birds (Olson, 1987; see also AOU, 1997). Emending and correcting citations continues, particularly with the dating of older works, e.g. Banks & Browning (1979), Browning & Monroe (1991), Poggi (1996) and Wheeler (1998). Changes to dates of citations are readily accepted where necessary (e.g. Schodde & Mason, 1997), yet while clarifying inconsistencies, they conflict with those already published in standard references. Should we suppress date corrections because of this conflict? Schodde & Bock imply such a necessity, particularly if a species subsequently has been 'gazetted by legislation', in the case of *Psephotus chrysopterygius*, but this change does not affect its protection under law. As to standard references cited by Schodde & Bock, the *Catalogue of Birds in the British Museum*, long out of date, was based on the 12th edition of Linnaeus [1766], not the 10th [1758], as now. The *Catalogue* is also a source of numerous emendations to established names on the grounds of purism, a practice no longer accepted. Peters's *Check-list of Birds of the World*, our current standard reference (Bock, 1990), nevertheless has instances of erroneous and confused citations and dates, incorrect synonymies, overlooked subspecies and even a name where the citation could not be found (but see Mees, 1986, p. 147). However, such necessary changes are, like our findings and those for North American birds, a very small proportion of the total. A number of citations in standard references are incorrect for other reasons. For example, the original name for the Sooty Albatross *Diomedea* [= *Phoebastria*] *fusca* is cited to Hilsenberg (1822), but if one checks the quoted source, one will find that the name actually appeared earlier in a German newspaper and the standard citation is merely an abstract of it. A further problem with many original citations is that they contain no information relevant to the subsequent acceptance of a taxon. For example, *Geophaps scripta peninsulae*, named in 1922, was not correctly diagnosed for 60 years (Frith, 1982). A more unusual example is the case of *Corvus mellori*, a name

proposed as a subspecies in 1912 and subsequently applied to a new species identified in 1967 because the type specimen of *mellori* (since lost) apparently belonged to it. Mayr (1971) considered the taxon to be a new species even though 'our queer rules of nomenclature' required the application of a name whose author 'did not appreciate at all the distinctness of this bird'.

11. Schodde & Bock accuse Olson and us of shoddy research in relation to Bonaparte (1855). First of all, Schodde & Bock misquote the name, it should read '*Somateria v.-nigrum*' (Bonaparte included the hyphen). Bonaparte's discussion is indeed anecdotal but the young bird quoted from his account by Schodde & Bock relates to one shown to Bonaparte by a 'M. Hardy, de Dieppe' from Hardy's private collection. Bonaparte then links his remarks on this specimen to several specimens, and drawings made before they were collected, seen in London with Gray at the British Museum. He then indicated that he agreed with Gray that in imitation of a name used for a butterfly by Linnaeus, the distinctive marking of this new species could be represented by *Somateria v.-nigrum*. Bonaparte clearly linked the distinctive new name, based on the duck's most diagnostic character, to the British Museum type material. We consider the name identifiable from Bonaparte (1855). On the matter of interpreting these remarks as joint authorship of Bonaparte & Gray, Bonaparte gave an explicit example in his preceding paragraph where we find '*Xylocota jamesoni*, Jard. et Bp.'. Yet if one turns to standard references (Peters, 1934; Hellmayr & Conover, 1948) this joint attribution is indicated in quotation marks but authorship is credited solely to Bonaparte. If the conclusion of Schodde & Bock is accepted, then there are literally hundreds of cases where authorship needs to be emended in the citations of original sources of names. Such an action would not conflict with the provisions of Art. 50 of the Code.

12. The concluding comments of Schodde & Bock focus on changes to the sources of names as being of greater concern than any real effect our paper may have on nomenclatural stability. In our opinion, the argument that quoting an earlier source of a name vs. *PZS* obscures important details does not preclude use of an earlier valid publication of a name. The Code is concerned with the source of a name meeting the definition of a publication (Art. 8), not where it is published. The argument of the role of original citations as sources of information on type specimens is misleading not only because *PZS* does not always mention them (as with most of Gould's) but because there are a great number of examples of later type designations (e.g. Schodde & Mason, 1997). Moreover, Gould himself did not acknowledge his own earlier publication of many of his new names (Bruce & McAllan, 1991, p. 455).

13. We conclude that where established nomenclature may be affected by an unnecessary change of name or application of name, not the published source of the name, then suppression may be warranted. Otherwise, as in North America's case, we prefer the discussion and resolution of issues of nomenclature on a case by case basis with any need for formal suppression applied as sparingly as possible. We oppose the concept of blanket suppression, as proposed in Case 3044 by Schodde & Bock, and support a more reasoned approach where only specific cases requiring suppression are proposed.

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Systematic ornithology is indebted to Bruce and McAllan on two particular counts. First, at considerable effort, they sifted out and collated a raft of undiscovered first publications of bird names in several popular mid-18th century periodicals so comprehensively (Bruce & McAllan, 1991) that the Standing Committee on Ornithological Nomenclature (SCON) of the International Ornithological Congress could deal with their treatment quickly and effectively (Schodde & Bock, 1997). This course has now been opened to the International Commission on Zoological Nomenclature, by Case 3044 which the SCON (and we) commend.

Bruce and McAllan's second contribution is their present opposition (above) to Case 3044. Because of its tortuous nature, their argument exposes with glaring clarity the real consequences of opting instead for 'reasoned' case-by-case discussion and resolution of the names in question. It would embroil us in didactic word-games and protracted debates that could carry on for years and, apart from keeping key issues of nomenclature and source references for names in limbo, involve the Commission in up to 20 Opinions, and potentially many more. The prospect is daunting, and out of all proportion to the importance of the issue; quite frankly, Bruce and McAllan have 'lost the plot'.

In contrast, Case 3044, which has as its sole objective the maintenance of stability for the nomenclature and source references of 6 generic and 45 specific names, offers a simple, straight-forward single-Opinion solution: it clears the decks of the so-far unused names and references. Its grounds have already been covered and explained in detail by Schodde & Bock (1997, 1998) and need no further advocacy here. Moreover, its provisions are the preferred solution by the great majority of the SCON, and, we stress again, were passed without dissent at the Vienna meeting of the SCON at which both Bruce and McAllan were present.

Only the case of *Somateria v-nigrum* G.R. Gray needs revisiting because issues raised by Bruce and McAllan affect a recommendation of Case 3044. We have consulted two different copies of the paper in which Bonaparte (1855) first used the name, and in both it is spelled simply '*v.nigrum*', without the hyphen (cf. Bruce & McAllan). More importantly, we continue to find no explicit and unambiguous connection between the juvenile diagnosed by Bonaparte and the undescribed material in the British Museum named '*Somateria v.nigrum*'. Such ambiguity and

differences of interpretation are further reason for treating Gray's (1856) use of the name as the first available, as proposed in Case 3044.

In conclusion, Bruce and McAllan take us to task for not consulting them on the formulation of Case 3044 — but have obviously forgotten why.

At the meeting at which the SCON directed us to prepare the proposal, we asked them to do it. They refused, one of them commenting to the effect that they had done their part in digging up the unused names and now it was up to others to provide solutions.

Comment on the proposed conservation of usage of 15 mammal specific names based on wild species which are antedated by or contemporary with those based on domestic animals

(Case 3010; see BZN 53: 28–37, 125, 192–200, 286–288; 54: 119–129, 189; 55: 43–46, 119–120; 56: 72–73)

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1. Gentry, Clutton-Brock & Groves address a contentious issue and their recommendations have received much support, but the consequences of their application are still unclear. Their agenda obliges us to consider wild names to the exclusion of other issues. Yet beyond this restricted remit it raises questions which should be answered prior to adjudication on the application itself. Approval may otherwise amount to a *fait accompli*, leaving problems to be settled by further appeal to the Commission. The submission suggests that there is a majority usage which should override application of the Code; junior species names should be retained for populations which are regarded as conspecific with others, to which senior names are assigned. The Commission is effectively asked to rule that certain species-group names are to be applied to particular populations within taxa (hence restraining the subjective use of synonymy), without requesting a general ruling on their priority. The application is therefore unusual. In the guise of a nomenclatural ruling, it is eliciting a systematic decision from the Commission (see Gardner in BZN 54: 125–126). Doubtless the Commission will carefully consider whether it is appropriate to use its plenary powers in such a context.

2. The formal request 'that the name for each of the wild species' listed is not invalid by virtue of being antedated by a name based on a domestic form' does not specify that the wild names must be used in the form of binomina. A trinomen — for example *Bos taurus primigenius* — would be within the letter of the request, for the wild name would retain validity. Although this is not what Gentry et al. intend, it is the literal meaning of their formal request that must be addressed. Perhaps it requires revision.

3. The application has insufficient space to discuss each of the 15 taxa separately. Such different instances as *Camelus ferus* and *Canis lupus* are lumped together. Not all the species have experienced 'traditional' separate naming for wild and domestic forms. *Bos mutus*, *Camelus ferus*, *Bubalus arnee* and *Equus africanus* were foisted upon the scientific community as replacements for species names based on domestic